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Long-Term Forecasts in International Economics

By WILLIAM R. CLINE*

Eric Blair's forecast from the 1940's called for a 1984 international political economy with constant war between three global powers, designed to use up resources otherwise so abundant that the masses would be free to reflect on, and consequently depose, the bureaucratic elite (George Orwell, 1949). Orwell's forecast was wrong, although perhaps in part only because it was a self-negating prophecy: one that induces corrective measures.

Undoubtedly the most successful long-term economic forecast was Joseph's prediction to the Pharaoh of the fourteen-year agrarian business cycle. Others with less privileged information have fared less well. A founder of our own profession, Malthus, erroneously predicted long-term stagnation at the subsistence level, because he underestimated technical change (although for Sub-Saharan Africa and parts of South Asia, the Malthusian projection appears closer to the mark). This essay reviews long-term forecasts from the last several decades in the area of international economics, to see whether patterns can be distinguished in their success or failure.

I. Secular Terms of Trade

In the 1940's and 1950's, some prominent economists maintained the thesis of a secular decline in the terms of trade for raw materials. Ragnar Nurkse (1944) spoke of "chronic" depression in terms of trade for agricultural goods, and Raul Prebisch (1959) built a theoretical justification for import-substituting industrialization around the concept that unfavorable demand elasticities would condemn producers of raw materials to continu-

ously declining terms of trade if they did not diversify toward industry.

The record has shown that terms of trade have fluctuated but not declined secularly. Declining terms of trade may be shown by selecting a high base period (such as the Korean War years) and a low terminal period (such as 1982); rising terms of trade may be shown by choosing a low base (the 1930's) and a high terminal period (1973-74). Technical change has lowered real prices of industrial goods, tending to offset lower income elasticities for raw materials. And there may be demand asymmetries that cause much sharper price increases for raw materials during boom years than the more gradual declines during years of weak international growth.

It is difficult to judge whether the long-term forecast of declining terms of trade has on balance done policy damage. It probably induced excessive and inefficient industrialization in developing countries. But some countries have succeeded in using the resulting industrial base for subsequent exports on an internationally competitive basis. Moreover, there is at least some degree of self-negation in the forecast: the fact that so many developing country planners accepted it meant less investment in raw materials exports, and thus a more favorable trend in terms of trade, than otherwise would have occurred.

II. The Dollar Shortage

The record is less ambiguous on another popular long-term forecast of the early postwar period: that the dollar would be in chronic shortage. Defining the dollar shortage as a persistent tendency for the U.S. current account surplus to exceed its long-term capital outflow, Charles Kindleberger (1950) maintained that the shortage had existed since 1914 (except for the period of normalcy in 1922-28). He attributed the

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problem primarily to a tendency toward secular stagnation in the United States relative to the rest of the world (p. 170).

Writing in 1956, Donald MacDougall (1957) acknowledged that there had been a "striking improvement" in the dollar shortage problem since 1946 as the result of increased production and currency devaluations in Europe. Yet he warned that dollar shortage was likely to return because of periodic U.S. recession, the tendency of other countries to live beyond their means as they sought to emulate U.S. living standards, and the structural advantages in U.S. trade in manufactures. His central forecast was that the rest of the world would run annual deficits with the United States, rising from \$ $\frac{1}{2}$ billion to \$4 billion over a decade. Because this trend would exhaust reserves in the rest of the world, MacDougall predicted a currency crisis in "five or six years at the latest" (pp. 325–26).

Even as authors were devising structural explanations for the dollar shortage, however, it was disappearing. After a large surplus on basic balance of payments (current account plus long-term capital and aid) of \$3.8 billion in 1947 (24 percent of merchandise exports), the balance shifted to a deficit of \$3.4 billion (34 percent of exports) during the Korean War year of 1950, the very year Kindleberger's book was published. Although the deficit then moderated, over the six-year period of MacDougall's predicted dollar shortage crisis (1957–62), the U.S. basic balance was in deficit by an average of \$2.3 billion (12 percent of exports; Walter Salant et al., 1963, pp. 6, 278–80). By the turn of the decade Robert Triffin (1960) was pointing out that the ongoing U.S. deficit would undermine the gold-exchange system by removing credibility of U.S. conversion of dollars into gold.

It is unlikely that predictions of chronic dollar shortage caused much policy mischief, however. The policy implications—increased U.S. economic assistance abroad, intensified efforts at export development in Europe, and so forth—can only be judged to have been salutary. But the predictions do illustrate one of the principal risks of long-term forecasts:

the tendency to project the conditions of the past into the distant future.

III. Raw Materials Shortages

Richard Cooper (1975) has reviewed another long-term forecast: the projections in 1952 of the President's Materials Policy Commission (Paley Commission) on supply of raw materials to the U.S. economy, a report prompted by concern during Korean War scarcities. Cooper found that the Paley Commission projected excessive requirements of raw materials, with the mean forecast (among 24 mineral products) 46 percent above actual U.S. consumption in 1972. Moreover, the Paley Commission underpredicted U.S. growth at 2.8 percent annually instead of the actual 3.6 percent, for a total *GNP* shortfall of 20 percent by 1975, with underprediction evenly divided between population and productivity growth. Accordingly, its projections of raw materials use relative to *GNP* were even further off the mark. After finding that movements in relative prices could not explain the shortfall of minerals consumption from the forecast, Cooper concluded that the Paley Commission seriously underestimated technological change.

In one qualitative dimension, the Paley Commission was more successful: it correctly predicted that "It will pay the United States to import much more raw material in the future than in the past" (Vol. I, p. 12). Its projections expected the 1952 Korean War surge in import shares for copper, lead, and zinc to become permanent by 1975. But the authors missed the most important case of rising import dependence; they erroneously expected the nation still to be almost self-sufficient in oil by 1975, despite their unusually accurate forecast of total energy needs (Paley Commission, Vol. I, p. 22; Cooper, p. 241).

The Paley Commission projections illustrate two ways that projections can go wrong: inappropriate parameters in the projection model, and erroneous assumptions about the exogenous variables. (A third source of error would be inaccurate model

structure, as opposed to parameter values.) The Paley Commission's mistake on the exogenous variable, economic growth, was understandable following one decade of depression and another in which a common view was that peacetime demand would be slack. Less obvious, but actually more important for the error in the outcome, was parameter error. From Cooper's summary data, it may be inferred that the mean elasticity of minerals consumption with respect to *GNP* (among 24 products) was assumed to be 1.1, and the average 1.2 (excluding one outlier, manganese). In actual experience the elasticities turned out to be 0.74 and 0.85, respectively.

This divergence illustrates another aspect of long-term forecasting. It should be possible to inform the elements of the forecast by theory. In retrospect, theory could have suggested to the Paley Commission experts that technical change (as well as a secular rise in the share of services) would mean a less than unitary elasticity of raw materials inputs with respect to *GDP*. Although the Paley Commission stated that it had considered both factors (Vol. II, p. 112), its implicit elasticities indicate that insufficient weight was given to technological change in particular.

From a policy perspective, it is tempting to conclude that the projections were formulated to be on the safe side, in an environment of concern about security of access to raw materials. There is something to be said in favor of this direction of bias, especially if it leads to heightened awareness but not to costly and unnecessary remedial efforts.

IV. Balance of Payments

In 1963, the Brookings Institution published a projection of the U.S. balance of payments in 1968 (Salant et al.). The U.S. payments position and the dollar had come under pressure beginning in 1958, and the weak dollar was seen as a constraint on fiscal-monetary policies for full employment. An important policy question was whether the situation would be self-correcting. The projection effort was heroic, because the balance of payments concerns differences between trade (and financial) credits and debits, and even modest errors in the underlying

variables can yield large errors in the differences.

The Brookings team constructed a three-country model (United States, Europe, and rest of the world). The U.S. exports depended on Europe's income, its prices relative to U.S. export prices, and on rest of world export earnings. The U.S. imports depended on domestic income and prices relative to European export prices. The authors assumed slightly higher U.S. growth than for Europe (4.8 percent vs. 4.2 percent). But their critical assumption was that U.S. export prices would rise at only 0.5 percent, while Europe's would rise at 1.5 percent. Their rationale was that declining labor force growth in Europe would put upward pressure on wages (whether the guest workers were taken into account is unclear), and that European governments would not tolerate much unemployment to avoid inflation. Primarily because of improving U.S. price competitiveness, the authors concluded that the U.S. basic balance would shift from a deficit of \$0.85 billion in 1961 to a surplus of \$1.9 billion in 1968.

At least some commentators at the time suggested that by taking price increases as given for the United States but deriving higher price increases from expected wage increases in Europe, the authors were biasing the forecast toward optimism (John Williamson in Joint Economic Committee, 1963, pp. 851–59). But by far the major upset to the forecast was the Vietnam War, which caused a surge in U.S. demand and inflation. Actual export price increases averaged 2.0 percent for the United States and only 0.5 percent for Europe over 1963–68, reversing the projected relationship; and nominal U.S. *GNP* rose 46 percent instead of the projected 36 percent (International Monetary Fund, 1978). The actual outcome was a basic balance deficit of \$6.6 billion (Commerce Department, 1970, pp. 36–67), driven by large private and official long-term capital outflows. The projection's large surplus on goods and services (\$9.1 billion) failed to materialize (the actual figure was \$1.8 billion), leaving a large deficit instead of the projected basic balance surplus of \$1.9 billion (Salant et al., p. 216).

In an illustration of the difference between model error and error in assumptions on exogenous variables, Salant et al. acknowledged "...the possibility that the basic assumptions may be wrong—which is indeed a hazard, but one of forecasting, not of projecting..." (p. 227). And the underlying Brookings model appears not to have been seriously biased; for example, the model captured relatively greater response of imports to income in the United States than in Europe (p. 90), a stylized fact from later econometric work (Hendrik Houthakker and Stephen Magee, 1969). Nonetheless, it is perhaps too generous to grant total absolution for "forecasting" as opposed to "projection" (model as opposed to assumption) error, especially if the intended audience includes policymakers. It would have been more prudent to include as an alternative variant one in which U.S. inflation was significantly higher than that in Europe. Instead, the alternative considered permitted only modest changes (slightly lower U.S. growth, slower growth and lower inflation in Europe—but not faster U.S. export inflation than in the base case), yielding a small basic balance deficit for 1968 (\$0.6 billion).

In retrospect one also wonders whether what might be called the Norman Vincent Peale syndrome was involved: the temptation toward positive thinking in policy analysis. Rephrased as the Burt Lance principle ("if it ain't broke, don't fix it"), this approach is not necessarily wrong, and it may avoid costly measures that turn out to be unnecessary. But emphasizing the positive should be recognized as the opposite of a minimax strategy that errs in the direction of caution to avoid large damage. And to the extent that the Kennedy tax cut and, more seriously, the Johnson spending surge, were facilitated by upbeat forecasts on the balance of payments, the costs of error were potentially significant. In their defense, of course, the authors cannot be blamed for failing to predict the Vietnam War.

V. Oil

In 1970, then-Secretary of Labor George Schultz headed a government study on the

issue of eliminating oil import quotas (Cabinet Task Force, 1970). The group favored import liberalization for economic reasons and argued that any security risks from greater reliance on imported oil were modest. The authors projected a decline in U.S. oil prices from the protected level of \$3.30 per barrel to the world price of \$2.00 (1969 dollars). They expected these real prices to hold through 1980, or even weaken as new production came in from the Alaskan North Slope. They projected that liberalization would increase the share of imports in 1980 U.S. oil consumption from one-fourth to one-half. But they anticipated little security difficulty because fully half of imports could come from Canada and Latin America, and only one-third would come from more unstable Arab states (with one-eighth from "reliable" Iran).

The task force did not err greatly in its volume estimate for U.S. oil consumption in 1980 (at 19.3 million barrels per day compared with 17.1 mbpd actual). Projected imports were somewhat further above actual (9.8 mbpd vs. 6.9), as some adjustment to sharply higher prices occurred. However, the Western Hemisphere turned out to supply only about one-fifth of imports (with Canadian supply far below projection), while Arab states provided nearly one-half (Commerce Department, 1983, pp. 528, 724–25).

Along with most other observers, the Cabinet Task Force failed to predict the OPEC price revolution. The actual 1980 price of \$34 per barrel was 8 times the projected level in real terms. The uncanny feature of this projection failure is that the task force was aware of the risk from OPEC price boosts, but rejected the risk as minor. The Cabinet Task Force study acknowledged:

Producing countries have sought and will doubtless continue to seek to increase their oil revenue by raising their taxes and royalties.... Whether they succeed in creating an effective price-raising cartel will, however, be largely independent of our import policy. ...[T]he world market seems likely to be more competitive in the future than in the past because the growing number and diversity of producing coun-

tries and companies make it even more difficult to organize and enforce a cartel. [p. 21]

Here the Norman Vincent Peale syndrome was, in retrospect, rampant. Moreover, the analytical framework of the authors based expected price on marginal cost of production and distribution, which they expected to be declining. Yet oil today is priced by the largest suppliers not at marginal production cost but at a reservation price, just as holders of gold bullion do not sell it for the marginal cost of removal from their vaults. Moreover, the task force seems to have been unaware that even as it was writing a historic change was taking place: U.S. proven reserves began to decline substantially (Commerce Department, 1983, p. 730). And the authors made the same mistake repeated frequently by others later in underestimating the cost of "backstop technology," anticipating that synthetic oil from shale could be produced at \$3.00 per barrel (p. 55).

Perhaps most seriously, the authors did not consider the interaction between their own policy recommendation and the likelihood of OPEC cartel action. Their projections indicated that, with liberalization, U.S. oil imports would surge from 2.5 mbpd in 1968 to 9.8 mbpd in 1980 (reaching 5 mbpd even without liberalization). Yet they treated the United States as a marginal importer and the world supply of oil as infinitely elastic. In fact, U.S. imports rose briskly to a peak of 8.5 mbpd in 1979. This increment amounted to perhaps one-fourth of OPEC export capacity. Rising U.S. imports almost certainly strengthened the demand conditions for the sharp OPEC price increases of 1973-74 and 1979-80. Moreover, although the authors were aware of another underpinning of potential OPEC power, low price elasticity of demand (the report used an elasticity of 0.1), they failed to draw the conclusion of considerable risk of a sharp price runup through coordinated OPEC action.

The Cabinet Task Force advocated eliminating oil import quotas because liberalization would save consumers \$5 billion annually (and provide net welfare gains of \$1.5 to \$2 billion annually). In the event, political

power of U.S. oil producers delayed liberalization until the mid-1970's. Actual costs to U.S. consumers from the increase in world oil prices by 1980 was approximately \$90 billion annually at 1969 prices.¹ Thus, if the probability was 5.6 percent (\$5 billion/\$90 billion) or higher that the elimination of U.S. oil import protection would ensure viability of the OPEC price revolution (by strengthening demand for OPEC exports), the recommendation to liberalize was a mistake. The odds are that this probability was at least this high, and in retrospect it would have been better to continue oil protection, although switching from a quota to a tariff would have transferred rents from foreign producers to the U.S. government.

VI. Other Examples

In the early 1970's steel producers around the world adopted major expansion programs, encouraged by temporary shortages in the 1973 boom. Within a decade there was massive excess capacity in world steel and the principal policy question was how to phase down production capacity. In the late 1970's, developing countries borrowed, and foreign banks lent, heavily on the basis of the implicit long-term forecast that the real interest rate would remain low or negative and export growth would remain rapid. By 1982 the U.S. macro policy mix and global recession thwarted both predictions and the debt crisis resulted.

In 1978, the World Bank issued its first *World Development Report* (World Bank 1978). The authors predicted that, after disappointing growth of 2.8 percent annually in 1970-75, the industrial countries would grow at 4.2 percent in 1975-85. The developing countries, which had managed to continue high growth of 5.9 percent in 1970-75 (in part by borrowing in response to the oil shock), would continue their growth at 5.7 percent in 1975-85 (p. 27). The actual record of growth in 1975-84 turned out to be 2.9

¹Applying the *GDP* Deflator to \$34 per barrel, subtracting \$3.30 per barrel as the comparison price, and multiplying by 19 mbpd projected consumption.

percent annually for industrial countries and 3.2 percent for developing countries (IMF, 1984, pp. 169–70). The report did not anticipate the second oil shock, the concerted contractionary policy response to it, and the worst global recession since the 1930s.

VII. Lessons

A broad implication of this review is that long-term forecasts are rarely right. One might have expected that by random chance at least one-half of the forecasts would turn out to be correct in at least qualitative terms. Yet the sample here is not (intentionally) biased toward historical mistakes.² It is possible that an asymmetry exists between successful and unsuccessful long-term forecasts. Both types come in two varieties: adverse and favorable. A successful (accurate) forecast that is adverse may become self-negating as public and private actors take corrective measures; one that is favorable may generate market responses (to profit opportunities) that also reduce its *ex post* validity. There are no corresponding mechanisms for turning bad forecasts into good ones, except random chance. A simpler interpretation, of course, is that long-term forecasts are usually wrong because they are difficult to make. Despite their difficulty, they cannot be avoided; anyone planning for retirement at least implicitly makes them, corporations need them, and governments must make choices that require them.

Long-term forecasters should avoid the tendency merely to extrapolate past experience, a pattern present in nearly all of the forecasts considered here. Attention to likely structural changes in the future, especially technological change, can help avoid this pitfall. In modeling terms, care must be used

not only in model structure, but also in changes from past trends with regard to assumptions on future values of exogenous variables and choice of parameter values. Where possible, theory should be coupled with empirical experience for improved results in all three areas.

Forecasters can take some solace from the likelihood that, in a good forecast, the basic outcome may be predicted correctly even if not all of the underlying variables turn out as expected. If errors are randomly distributed, the chances are that the value of the dependent variable in a multivariate model will not be far wrong, because departures in one direction for some exogenous variables will tend to be offset by departures in the other direction for others (see my 1984 study, p. 169). Finally, policy forecasting should avoid at least two temptations: to corroborate the view of the established regime, and to err systematically on the side of optimism. Ideally, sensitivity analysis should be included, with an assessment of the probabilities of alternative outcomes, and attention to the policy costs of premising action on the wrong one.

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²Surprisingly, one of the more heavily criticized projections—secular decline in terms of trade—turns out to have been among the more accurate, if allowance is made for the influence of self-negation. Note that a proper test of the hypothesis would involve a simulation of what the terms of trade would have been in the absence of structural transformation away from raw materials exports by policy intervention in developing countries.

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